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PROBLEMS ASSOCIATED WITH THE STUDY OF CORAL REEFS. III

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Uppgrowing Reefs in a Rising Ocean.—Are we then to regard Darwin's theory of subsidence as completely established? Not yet, for there are two alternative theories that have not been mentioned. It has already been pointed out that all the visible features of sea-level reefs themselves, apart from the neighboring volcanic islands, can be equally well explained by any one of some eight or nine hypotheses, provided that the postulates of the hypotheses are accepted; and that appeal must therefore be made to some associated problem in order to find a crucial test by which the true theory can be selected. Let it now be recognized that all the features of barrier reefs and of their embayed central islands also, which according to Darwin's theory are explained by the subsidence of the reef foundation beneath a stationary ocean surface, can be equally well explained by supposing the ocean surface to rise over a stationary reef foundation, as in Fig. 30. It is only by appeal to certain rather recondite associated problems regarding the rest of the world that this second possibility can be excluded, and the first fully justified.

In discussing this aspect of the problem we must recall at the outset that an adequate theory of coral reefs has to account, not only for the last touches given to existing sea-level reefs, but also for their relatively remote beginning; that it has to explain not only sea-level reefs, but also elevated reefs, formed in an earlier epoch and now standing hundreds of feet above sea-level; that it has to provide reasonable conditions for the great submergence indicated by the deep embayments of Kandá-vu, Tahaa, and many other islands; that it has to account for the occurrence of small remnants of large volcanic islands within large reefs; and that it has to explain the heavy limestones in such uplifted atolls as are found in the Loyalty islands and elsewhere, as well as the thin terracing reefs that are uplifted in the New Hebrides. In short, an adequate theory of coral reefs involves great and small terrestrial changes such as will cause great and small submergences and emergences at different dates in the later ages of the earth's history. If we account for all such changes, not by local subsidence of the reef foundations, but by changes of the sea surface caused by deformation of some

other parts of the sea floor, we must postulate repeated crustal movements of so enormous a measure that they become improbable to the point of incredibility; for it must be remembered that whenever the sea surface is to be raised 1,000 feet by upheaval of a part, such as a tenth, of the sea floor, that part must be upheaved at least 10,000 feet; and more than 10,000 if, as is very probable, its upheaval is accompanied by a contemporaneous depression of some neighboring part. It is therefore consistent with terrestrial economics to account for the great submergence which coral reefs demand by subsidence of the needed amount in the area where and at the time when the reefs are formed.

Furthermore, if the submergence indicated by coral reefs and their associated islands be explained by a rise of sea level, let it be remembered that this requires the submergence to be everywhere—on all continents and all islands—the same in rate, date, and amount, except where local crustal movements introduce variety into its measure. In view of the apparent variety in the date and amount of submergence by which existing embayments of insular and continental shorelines have been formed, the appeal to local movements as a cause of the variety may be so frequent that such movements will become the dominant control of submergence, each in its own region. Indeed the explanation of all coral reefs by a rise of sea level recalls the explanation given by Suess, about thirty years ago, for all high-standing atolls by a general depression of sea level; as soon as the diversity in the altitude of such atolls is recognized, their position must of course be explained by diversity of crustal movement rather than by a uniform change of sea level: so, I believe, when the submerged embayments of the world's coast lines in coral-reef regions and elsewhere are attentively examined, and when the emerged coastal plains of certain continental borders are closely studied, many diversities in the rate, date, and amount of their submergence or emergence will be discovered. Such diversity will then be best explained by local crustal movements, and world-wide changes of sea level will be given relatively small values.

But let me state explicitly that it would be highly illogical to exclude so manifestly possible a factor as a universal change of sea level from all share in submerging shoreline embayments or in laying bare strips of coastal plains; it is because any change in sea level must be smaller than the uplift or depression of the sea floor which causes the change in the same proportion that the uplifted or depressed area is smaller than the total ocean area, and because the sea integrates all the positive and negative changes which various local movements tend to produce in its surface level, that it seems most reasonable to explain regional submergence, long continued in time and great in amount, such as that involved in the origin of many coral reefs, chiefly by subsidence in the region and at the time concerned.

The Glacial-Control Theory of Coral Reefs.—There is one phase of the theory of a rising ocean, pointed out by several writers in the last thirty or forty years and lately discussed more fully by another, that demands further examination. This is the idea that the drowned-valley embayments, which we have been taking as signs of subsidence, have been submerged by the rise of the ocean surface as the water, which was abstracted to form the continental ice sheets of the Glacial period, was returned to the ocean when the ice sheets were melted in the milder climate of Postglacial time. This in its fully developed form has been called the Glacial-control theory, and its discussion evidently involves many associated problems besides those already mentioned. Its essential postulates and processes are as follows: Coral-reef flats are supposed to have been formed around or upon still-standing foundations in Preglacial time, hence presumably for the most part by outgrowth; the ancestors of atolls, now the commonest kind of reefs, were formed around volcanic islands that had stood still long enough to be worn down by subaerial erosion nearly to sea level or by marine abrasion a little lower; the ancestors of barrier reefs were similarly formed around islands that are not old enough to have been worn down; lagoons were shallow or wanting, because solution is not an adequate cause for them. As the Glacial period came on, the surface of the ocean was lowered by the imprisonment of a large volume of water as ice on the lands; it has been calculated that the lowering of the entire ocean thus caused was between 200 and 300 feet. The ocean was then chilled as well as lowered, and thereby the corals of most reefs were killed. The waves of the lowered sea attacked the undefended flanks of the dead reefs and cut them back in flat platforms. Next when the climate became milder and the ocean surface rose again, reefs were reestablished and grew upwards on the platform edges of the earlier reef flats, and thus the barrier and atoll reefs of to-day have been built up around the lagoons that they enclose. It thus appears that the Glacial-control theory accounts only for the reef rims and the lagoons of atolls and barriers, and that the great undermass of the reefs is explained according to the theory of outgrowing reefs on still-standing foundations, but without the action of solution in the excavation of the lagoon.

All the causes here mentioned appear to be true in quality, but their quantitative value is uncertain. The oceans must have been lowered and cooled during the Glacial period, but no one knows how much. The corals of reefs that had grown in Preglacial time on the northern and southern borders of the coral zone, as around the Hawaiian islands, were presumably killed; but it is impossible to say, *a priori*, whether the corals were killed through the middle of the torrid coral zone as well. Dead reefs must have been attacked by the sea below their crown, but who can say how far they were cut away? Live reefs would not

have been cut away. Additional factors are introduced by the complexity of the Glacial period, for it includes several Glacial epochs of different intensities separated by Interglacial epochs of different durations; and no one yet knows how to date the history of coral-reef islands in terms of Glacial chronology. The quantities involved in the Glacial-control theory are therefore of uncertain value.

Uncertainties of the Glacial-Control Theory.—There are two other groups of uncertainties in the Glacial-control theory, as it has thus far been set forth. The first arises from the improbable nature of one of its fundamental postulates; the second, from the over-long series of deductive steps—not always sharply defined—for which no sufficient verification has been provided by confronting them with observable facts. The improbable fundamental postulate is that the ocean bottom did not subside in the coral-reef region for a long period of Preglacial and Glacial time. No reasons are offered in support of this singular assumption; yet it is held to be so true that ancient volcanic islands, possibly formed as long ago as in Paleozoic time, are believed to have suffered flat truncation by erosion and abrasion, down to or a little below sea level, and that the flat surface of truncation remained close to sea level, without subsidence, until the Glacial period! Is it not venturesome to give subsidence the special value of zero for so long a time in the coral-reef region of an ocean, the bottom of which has suffered repeated elevations, as shown by uplifted coral reefs, and has suffered, in its Australasian area, demonstrable deformation, with many ups and downs, as shown by the isolated remains of former continental lands? And is it really necessary to make subsidence zero for a long time and over a large area in a theory in which the occurrence of subsidence now and again at a slow rate or by starts and stops is not at all incompatible with the various processes of Glacial control, even though it would affect the value of their results? Yet as stated by the latest expositor of the Glacial-control theory, this unnecessary and venturesome postulate is practically insisted upon, and the theory is therein made to resemble the several other theories that arbitrarily postulate still-standing islands, and thus place themselves in opposition to Darwin's more general theory of subsidence: "more general," because subsidence can have all grades of value from its maximum down to zero, and because prevailing subsidence may be interrupted by still-stand pauses or even temporarily reversed into uplift, as Darwin clearly stated; but still-stand theories are based on a rigidly fixed postulate of immobility, as far as subsidence is concerned.

Among the unverified deductive steps in the Glacial-control theory, we may note in particular the Preglacial truncation of numerous still-standing volcanic islands at sea level or a little lower; the Preglacial formation of numerous marginal reefs by outward growth on an ad-

vancing talus of coral débris around still-standing truncated flats or shoals; the reduction of ocean-surface temperature in the coral zone during the Glacial period sufficiently to kill the corals of nearly all reefs; the complete abrasion of even the broadest atolls during the lowered sea-stands of the Glacial period; the unconformable upgrowth of new atoll reefs on the abraded platform of low Preglacial islands as the sea rises; and again, the Preglacial formation of outgrowing encircling reefs around younger, mountainous, still-standing, non-embayed volcanic islands; the reduction of these reefs to platforms by abrasion during the lowered sea-stands, but without significant encroachment upon their central islands; the erosion of the deeper parts of the central-island valleys, now drowned in embayments, during the lower sea-stands of the Glacial period, but to no greater depth than the level of the lowered sea; the sufficient widening of the deepened valleys by the slow process of valley-side weathering during the lowered sea-stands, so that, when drowned, the valleys shall contain the well-opened embayments now visible; and the unconformable upgrowth of new barrier reefs on the abraded platforms with the rise of the sea.

No sufficient verification is provided for this elaborate series of deductions. Not a single example of a truncated volcano is known in the coral seas. Not a single example of a recently uplifted atoll is known to have an abraded volcanic area in the center of its lagoon-plain. Not a single example of an uplifted atoll is known to consist in its underpart of the steeply inclined talus layers composed chiefly of coral débris, such as must here be supposed for the great undermass. Not a single example of a recently uplifted atoll or barrier reef is known in which the reef wall stands unconformably on a flat platform abraded across a series of slanting talus layers largely formed of coral débris, as must be the case under the Glacial-control theory. True, an attempted verification of the abrasion of Preglacial atolls during the lowered sea-stands of the Glacial period has been offered in a table of the depths of atoll lagoons; but the measures of depth show no such accordance as the theory demands. The maximum depths of the lagoons vary through a large fraction of the supposed depth of the abraded platform; the mean depth of the deeper parts shows a similar variation. Such inconstancy of lagoon depth can be explained only by supposing that the atoll platforms were abraded at various levels, for which no good reason is assigned; or by supposing that the platforms, after being abraded at a uniform depth, have been irregularly covered by Postglacial lagoon deposits. The latter supposition is extremely probable, as far as the occurrence of lagoon deposits is concerned; but if accepted it leaves the platforms without verification. It may be truly said, in reply to this and the preceding paragraph, that all theories of coral reefs, the subsidence theory as well as the rest, involve unverified deductions; but as far as I

can see, the subsidence theory is less deficient in this respect than any other.

In view of the insufficient attention that, as thus appears, has been addressed to the matter of confrontation and verification in the Glacial control theory, in the form in which it has thus far been set forth, it seems desirable to give closer examination to that phase of the problem; not with the intention of making up the deficiencies in the statements of its expositors, for that is their own affair; but with the wish of finding for oneself definite grounds which shall warrant a decision in favor of the theory or against it; for it is the duty of every investigator to make himself personally responsible for the critical discussion of every theory that has been offered in solution of his problem; he must not

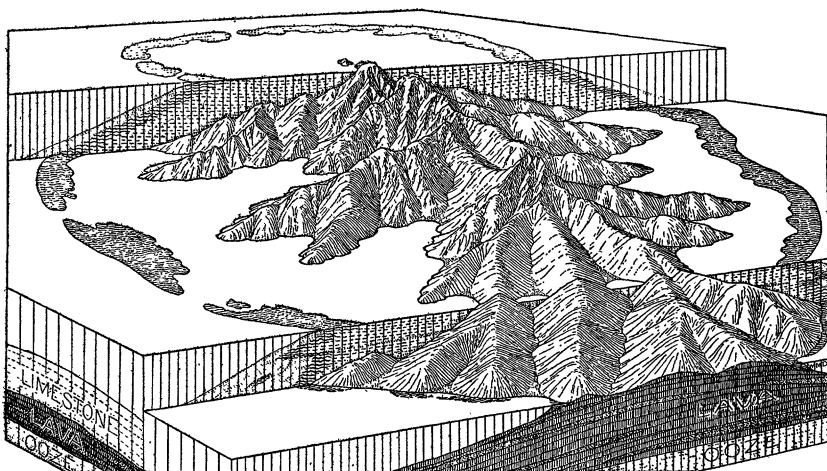


FIG. 30. BLOCK DIAGRAM OF A STILL-STANDING VOLCANIC ISLAND IN A RISING OCEAN. In the narrow foreground block, the island has a simple shore line with a discontinuous fringing reef; in the middle block, the rising ocean has half-submerged the island, giving it an elaborately embayed shore line, and the fringing reef has developed into a barrier reef by up-growth; in the background block, the rising ocean has almost submerged the central island, and the barrier reef has almost become an atoll. The first up-growth of the fringing reef, as shown on the front face of the middle block, is drawn nearly vertical, because the amount of waste taken from it to form its short external talus was then small: the effect of a pause in the rise of the ocean is shown in the horizontal out-growth at mid-height in this section. The later up-growth of the barrier reef, as shown on the front face of the background block, is drawn inclining inward, because a large amount of reef waste is needed to form the greatly prolonged talus, which now begins at a greater height and must extend down nearer the base of the volcanic cone. Compare this figure with Fig. 16.

throw off that responsibility, or leave it wholly to the inventors of other theories than the one to which his judgment inclines. The first step to be taken in meeting this responsibility with regard to the Glacial-control theory is to define more sharply certain consequences to which it leads; and in doing so it might be well, as above intimated, to give special attention to reefs near the border of the coral-reef zone; but as the

only reefs so located that I visited were on Oahu in Hawaii and along the coast of Queensland in Australia, this method of testing the theory will not be pursued for the present. The reefs of the torrid zone, of which I saw more abundant examples, will be examined instead; and for this purpose several tentative assumptions will be made, from each of which certain essential consequences will be deduced, in order to confront them with the facts.

Special Consequences of the Glacial-Control Theory.—For example, sector *E*, Fig. 31, represents a rather narrow Preglacial reef plain,

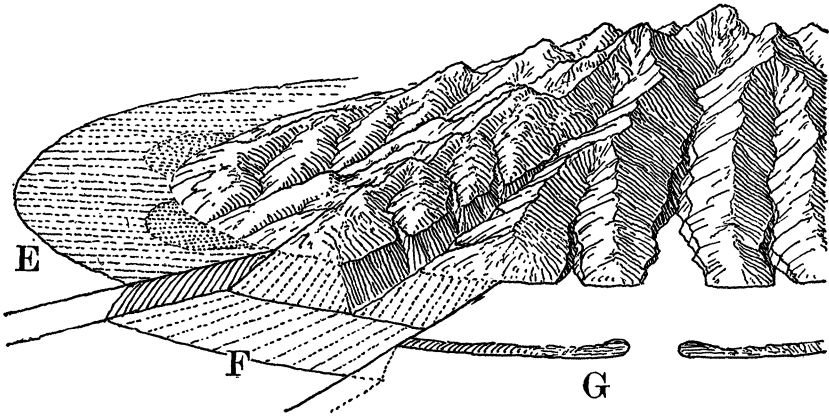


FIG. 31. DIAGRAM OF SUCCESSIVE STAGES OF REEF FORMATION, as deduced from the Glacial-control theory, on the supposition of a relatively short period of lowered ocean level.

with no lagoon, bordering a dissected, still-standing volcanic island of non-embayed shoreline and salient deltas. In sector *F*, we see the abraded platform cut by the waves of a lowered and chilled ocean, which did not, however, work long enough to becliff the volcanic island; and also a young valley incised with reference to the lowered sea level in the floor of a Preglacial valley. In sector *G*, the sea has risen and warmed, a barrier reef has been built up enclosing a lagoon, and the young valley is embayed. A detailed consequence of these changes is an "edge" where the steep walls of the young valley cut the gentler side-slopes of the Preglacial valley; this edge ought to begin close to sea level on either side of the new embayment near the former margin of the island, and gradually ascend inland beyond the embayment to the head of the young valley. No such edges, no such "valley-in-valley" forms were found in any one of the hundreds of partly drowned valleys that I saw in the Pacific; hence a second assumption must be tried. Sector *E*, Fig. 32, shows the Preglacial condition again; sector *F* represents the work accomplished during a longer time of lowered sea level than before, a time long enough to allow the widening of the new-cut valleys so that

all trace of the Preglacial valleys should vanish, even to their heads; but in so long a time as that, the sea would surely cut away not only all the Preglacial reef-plain, but part of the volcanic island as well; then, after the sea rises as in sector *G*, the spur ends between the wide embayments should be truncated, and the steep faces of their cliffs would plunge below sea-level. Cliffs of this kind ought to be specially well developed around the younger volcanic islands, which in Preglacial times had only narrow fringing reefs or no reefs.

But spur-end cliffs do not occur on such islands: hence the corals of their reefs cannot have been killed. Perhaps the drowned spur-end cliffs of Tahiti, figured above, are the work of abrasion during the lowered sea-stands of the Glacial period; but if they are, Tahiti is of so ex-

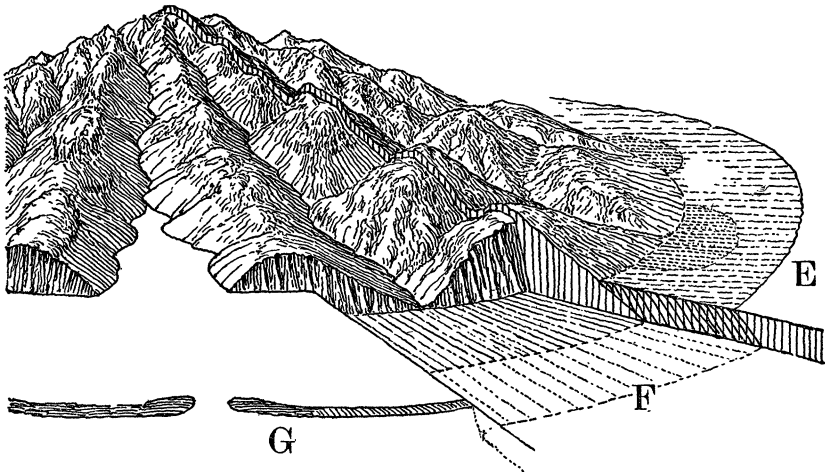


FIG. 32. DIAGRAM OF SUCCESSIVE STAGES OF REEF FORMATION, as deduced from the glacial-control theory, on the supposition of a relatively long period of lowered ocean level.

ceptional a form in this respect that its cliffs prove too much for other islands. The spur ends in the other members of the Society group and elsewhere are as a rule not cut off in cliffs; or if a little becliffed, a shallow platform extends forward from the cliff base, showing that the cliff has been recently cut at present sea level. Hence in order to explain the prevalently tapering forms of non-cliffed or little cliffed spurs, it must be supposed that the corals of their enclosing reefs were not killed while the sea was lowered, and thus an essential half of the Glacial-control theory, as it has been recently propounded, is excluded.

However, even if the corals were not killed and the reefs were not cut away, the valleys of the central islands must have been deepened while their streams ran down to a lowered sea level. But is it reasonable to suppose that the lowered sea level of the Glacial period en-

dured long enough to allow not alone the incision of narrow valleys by small streams, but also the widening of the valleys by slow weathering to the observed width of the actual embayments, some of which are one or two miles across? The simplest test I have found for this question lies in a comparison of the drowned-valley embayments of the volcanic islands in the Pacific with the valleys of certain dissected volcanoes in central France. According to French observers, those volcanoes are of Preglacial origin, and they have suffered glaciation more than once; but their valleys, even where enlarged by glacial erosion, have not consumed so much of their initial form as has been consumed in the embayments of Tahaa or of Borabora in the Society group, or of Ka-ndá-vu in the Fiji group, or of Rarotonga in the Cook group, or of Oahu in Hawaii; nor so much as has been consumed in the embayments of New Caledonia or of Queensland, which consist of continental, not of volcanic rocks. The valleys of the embayments in these islands are too wide to have been cut during so relatively short a time as the Glacial period; hence I am driven to think that the processes of the Glacial-control theory, acting alone, are incompetent to produce the observed results. Nevertheless the level of the sea must have been lowered during each epoch of the Glacial period, and must have risen in each Inter-glacial epoch as well as in the present Postglacial epoch. How can these undeniable changes be best included in a general theory of coral reefs?

Combination of Subsidence and Glacial Control.—The element of the Glacial-control theory which seems to me least reasonable is the postulate of still-standing reef foundations. Fortunately that postulate is unessential. Let us therefore see whether the Glacial-control processes and the subsidence processes cannot work harmoniously

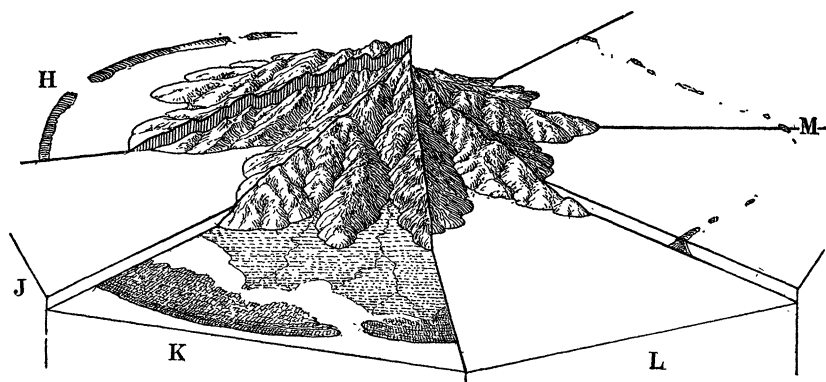


FIG. 33. DIAGRAM OF SUCCESSIVE STAGES OF REEF FORMATION, as deduced from a combination of the glacial-control theory with Darwin's theory of up-growth during subsidence.

together. Sector *H*, Fig. 33, represents a barrier reef developed during Preglacial subsidence in a region where Glacial cooling, when it comes,

shall not be sufficient to kill the corals. Sector *J* shows the effect of continued subsidence without upgrowth in an unchanging ocean. Sector *K* shows the effect of neutralizing subsidence by the contemporaneous lowering of the ocean surface as the ocean water is withdrawn to form continental ice sheets; that is, if the island sinks at about the rate that the ocean is lowered, a relative still-stand is the result for the time being; and during a long enough still-stand a broad and mature reef plain will be developed by the outgrowth of the reef itself, by the in-wash of its waste, and by the forward growth of deltas which will fill the former embayments and advance into the lagoon, as in Sector *K*. Now if subsidence continue during a maximum of glaciation, a critical condition will be reached when the ice sheets begin to melt and the ocean begins to rise, as in sector *L*, for the joint effect of ocean rise and subsidence will be a doubly rapid submergence, sector *M*, as a result of which the upgrowth of the reef may not keep pace with the rise of sea level, and the reef may become discontinuous; its renewed growth may even be stopped if it is "drowned" by too rapid submergence. Perhaps the discontinuity of certain reefs that rise, as has recently been pointed out, from submerged platforms, but not necessarily on their outer edge, may be explained in this way. In any case, I believe that some combination of regional subsidence with Glacial changes of sea level—or with changes of sea level caused by movements of the sea bottom—is worthy of careful consideration as being probably nearer the truth than either process taken alone: but of the several processes, subsidence in the coral-reef areas seems to me to have acted through a longer time and to have played by far the greater part in the development of coral reefs.

Other Hypotheses Regarding Coral Reefs.—Various other hypotheses have been proposed to account for coral reefs. Some of the early explorers explained reefs as determined by the instinct of the "coral insects"; others regarded them as built upon the rims of submarine craters; one long-practised student of the problem in more recent years suggested that an extensive system of barrier reefs near one of the larger Fiji islands may have grown up, independently of subsidence, from submarine lava flows; an observer in Samoa suggested that certain reefs in that island group have been determined by submarine hot springs; a widely experienced traveller in the Pacific suggested that many atoll-reefs are of small thickness, formed around uplifted and worn-down limestone masses of unexplained origin; a less experienced observer accounts for the outline of atolls largely by the action of wind-driven waves. These suggestions all represent conceivable possibilities; but they are not supported by sufficient evidence to show that they correspond to geological realities. The first of the six hypotheses is only a relic of an earlier philosophy; the second was generally accepted

till Darwin showed its impossibility; the third entirely overlooks the shore-line evidence of strong submergence following submature dissection; the fourth hypothesis makes the gratuitous assumption that submarine hot springs exist in requisite number and position; the fifth hypothesis includes no adequate explanation of the assumed limestone masses, although, where little dissected remnants of such masses are found, they have every appearance of being atolls of an earlier date; the sixth hypothesis exaggerates the action of a true cause, for it overlooks the usually concentric arrangement of central islands within their barrier reefs, as shown in Fig. 19; this arrangement demonstrates the action of winds and waves to be of secondary importance in determining reef outlines. Need these hypotheses be further considered?

Conclusion.—To what conclusion is an inquirer led by this study of coral reefs? At least to the certain conclusion that the origin of coral reefs, like the origin of many other geological features, is not susceptible of absolute demonstration. All that can be hoped for in problems relating to the past origin of present features, such as synclinal ridges in mountains of folded structure, hanging lateral valleys in formerly glaciated mountains, "faulted" strata, "metamorphosed" rocks, and coral reefs, is a highly probable explanation. It is true that geologists are accustomed to regard various highly probable explanations as having the same order of verity as directly observed facts: for who doubts that faulted strata, for example, have been displaced from their original continuity, or that fossils are of organic origin? Nevertheless, these universally accepted conclusions are nothing more than highly probable inferences. Why, then, are they universally accepted? Because the experience of many geologists through many years in many places gives every reason to accept them as correct, and as yet no reason to reject them.

No theory of coral reefs at present reaches so high a degree of probable correctness as to be universally accepted. It is true that most geologists of fifty or sixty years ago thought the coral-reef problem was fully explained by Darwin's theory of upgrowth during subsidence; but geological thought at that time was not so critical as it is now regarding the acceptance of hypotheses; and moreover the problem of coral reefs had not then been enriched by the invention of several alternative hypotheses. There was at that time practically only one theory of coral reefs before geologists worthy of consideration; it explained the things that it had been invented to explain, and that was deemed sufficient to ensure its correctness and to warrant its acceptance. In the last thirty-five years certain additional theories of coral reefs have been brought forward, all the theories have been subjected to closer scrutiny than before, and one or another of them has been accepted with more or less confidence by a less or greater number of geologists; but the majority of geologists,

disconcerted from the confident belief in Darwin's theory of subsidence that formerly prevailed, and demanding strong evidence before they accept any one of the several current theories, now have no definite views in the matter: they learn that the problem is "unsettled"; they have no opportunity of studying coral reefs themselves, and as a rule not time or inclination enough to make a close study of the work of those who have studied coral reefs. Such is the uncertain condition of the problem to-day. In view of this and as a summary of the fuller discussion on the preceding pages, the present writer offers the following statement of his own opinion regarding the several current theories as the result of much reading, observation, discussion and reflection during the past two years.

The origin of barrier reefs and atolls on still-standing islands more or less completely truncated by marine abrasion is contradicted by the features of the islands within barrier reefs and by the structure of uplifted reefs. The resurrection of this theory in recent years, after it had been shown by Darwin over 70 years ago to be incompetent as an explanation of barrier reefs, has not advanced the solution of the coral-reef problem.

The origin of atolls by upward and outward growth on still-standing submarine foundations capped with pelagic deposits is a manifest possibility, but it is not confirmed by any independent evidence. The theory is unfortunately limited by the unnecessary postulate of still-standing foundations. If this limitation be relaxed, and if, in view of the common occurrence of uplifted reefs in various parts of the Pacific, uplift is supposed to take place at a rate a little faster than the downward erosion of the uplifted mass and a little slower than the outward growth of the fringing reef, a conical coral island would result; the cone would be terraced if the uplift were intermittent; as erosion progressed, a volcanic nucleus would be disclosed. No such conical coral islands are known; hence uplift at the rate here suggested cannot have been commonly associated with the formation of atolls. But uplifted and little dissected atolls occur not infrequently as tabular islands; they show that rapid uplift sometimes occurs after the construction of an atoll is well advanced. Uplift therefore seems, when it acts, to be a more effective process than upbuilding.

If, on the other hand, in view of abundant evidence of subsidence that is found especially in the western part of the Pacific coral-reef area, intermittent subsidence is considered in association with the up- and out-growth theory of atolls, then this theory merges into Darwin's theory. If both uplift and subsidence are considered, it is evident that uplift is most favorable in the early stages and subsidence in the later stages of the process; for subsidence in the early stages would as a rule prevent the effective upbuilding of a submarine summit

by a pelagic capping, and uplift in the later stages would prevent the formation of normal atolls. Since normal atolls are to be counted by scores or hundreds, it seems, when all reasonable factors in their formation are considered, extremely improbable that many of them have been formed on submarine volcanoes that grew by eruption near enough the surface to be further built up to the coral limit by a pelagic capping, but did not grow by eruption above the ocean surface sufficiently to form enduring islands, and that, after their eruptive building had ceased, always stood still and never subsided. Indeed it would require extraordinary unanimity among pelagic volcanoes, in an ocean in which recent and subrecent eruptions are so numerous and in which signs of recent uplift and subsidence are so widespread and abundant, to build up a series of lofty cones from the deep ocean bottom, scores of which should approach close to the surface in regions now occupied by atolls, but none of which should form islands originally large enough to be still visible although much worn down, and none of which should suffer elevation or depression!

If more facts were known about atoll structures, some of the supposed possibilities above noted might be excluded; but as long as observation is limited to the surface reefs of atoll rings, their origin must remain a matter of speculation. Yet in view of what is known of other coral islands, the most reasonable result of speculation today must include subsidence as a factor in the making of many or most atolls.

The outgrowth of barrier reefs around still-standing islands, the excavation of their lagoons by solution, and the conversion of the barriers into atolls by the erosion and abrasion of their central islands is easily conceivable; but observation shows that lagoons are the seat of deposition, not of solution; no example of the almost-atoll stage, in which the central island of a barrier reef is reduced to a lowland with an alluvial rim, is known. The rigid fundamental postulate of still-standing reef foundations is contradicted by abundant evidence of changes of level, by the structure of certain uplifted reefs, and by the eroded surface of the volcanic or other rocks on which uplifted reefs rest. If the rigidity of this postulate is relaxed, and subsidence is allowed as demanded by various lines of evidence, a lagoon will be formed without the aid of solution, and the still-stand theory will be transformed into Darwin's theory.

The modification of the theory of outgrowth on still-standing foundations by the processes of the Glacial-control theory appears to be based on true factors, but unfortunately the quantitative value of these factors is unknown. The theory is ingeniously developed through a long series of deduced consequences, but it is arbitrarily limited by the unessential postulate of still-standing reef-foundations in the coral-reef region during a long period of time, and many of its consequences are,

as above noted, not confirmed by observation. The moderate abrasion of narrow-lagoon barrier reefs is, I believe, incompatible with the broad abrasion of large atolls; hence the corals of most reefs were not killed in the Glacial period. Many now-embayed valleys in barrier-reef islands are, as well as I can estimate, too wide to have been eroded during the Glacial period; hence their erosion must have been well begun at an earlier date when the islands stood at a greater altitude, from which they have since subsided. Thus the Glacial-control theory appears inadequate. If the unessential postulate of still-standing islands is omitted from the theory and the processes of Glacial-control are combined with those of Darwin's theory, as they well may be, the combination is a helpful one, especially along the margins of the coral zone.

It has been pointed out that all the consequences of Darwin's theory of upgrowth during subsidence may be accounted for by a contrasted theory, in which subsiding islands in an unchanging ocean are replaced by fixed islands in a rising ocean. It is entirely possible, nay, highly probable, that such replacement may have sometimes occurred; but it is extremely improbable that the replacement should be complete, for the following reasons. A moderate rise of ocean level over fixed coral-reef islands demands a great rise of a small part of the ocean bottom, or a moderate rise of a great part of the ocean bottom, always excepting the part where the fixed coral-reef islands stand; it demands also various movements of such other islands and continental borders as do not suffer submergence equally with the still-standing coral-reef islands: hence if the rising-ocean theory is to explain all coral reefs, it must demand that much or most of the earth's crust shall long be mobile, while the crust beneath the coral-reef islands must as long remain fixed. Such a demand is too absurd to be accepted. Moreover, if the rest of the earth's crust is so mobile, part of the ocean bottom outside of the fixed coral-reef region may sometimes sink, and the consequent lowering of the ocean's surface would be universally unfavorable to reef formation. Hence the rising-ocean theory can not be accepted as accounting for all coral reefs, and the part that it has played in accounting for any part of them must long remain uncertain. The uncertainty attending this theory is, indeed, so great that the theory is in danger of being discredited thereby: yet by nothing less than a world-wide observational study of coast lines can the uncertainty be resolved.

Darwin's theory of upgrowth during subsidence is not based on the rigid postulate of fixed reef foundations, but on the elastic postulate of a mobile earth's crust. It regards areas of subsidence as the most favorable for the growth of barrier reefs and atolls; it explicitly takes account of a possible variation in the amount of subsidence from place to place, and it as explicitly includes possible still-stand pauses and occasional moderate uplifts as interruptions in a prevailing subsidence.

Its processes are in no way inconsistent with those of the Glacial-control theory, for the two may be combined; its inferences as to extensive areas of ocean-bottom subsidence may be modified as new facts demand, without in the least invalidating its general value. True, the subsidence that it demands in certain areas is apparently to be measured in thousands of feet, and this seems formidable; yet objections to this demand are based much more on our ignorance than on our knowledge regarding the dynamics of the ocean floor. Hence Darwin's theory provides more general and more probable fundamental conditions for its following processes than are provided by any other theory. Furthermore, as far as present observation in the Pacific goes, nearly all the deduced consequences of the theory of upgrowth during subsidence are confirmed: unconformable contact of limestones in uplifted reefs on an eroded insular or continental foundation, instead of on a non-eroded foundation as is required by all the still-stand theories; observed or reasonably inferred large thickness of certain elevated reefs, instead of small thickness as required by the veneer theory; horizontal structure and rarity of fossil corals in the inner part of uplifted reef masses, instead of inclined structure and abundant fossil corals as the still-stand theories demand; embayed central islands within barrier reefs, the embayments being of such width and depth as no other but the very improbable rising-ocean theory can account for; steep but not becliffed islands of small size in the large lagoons of such barrier reefs as have almost reached the atoll stage; and lagoons of various depths, floored with accumulating deposits. So successful a confrontation of deduced consequences with observed facts is certainly very appealing.

Such is the array of evidence that has led me to regard Darwin's theory of upgrowing reefs on subsiding foundations as not only the most acceptable theory of coral reefs yet invented, but as with high probability the dominantly true theory of coral reefs. It is probably destined to be modified in subordinate measure as new facts come to light, and many new facts are without question still to be discovered; but it seems also destined to stand as the leading theory, even when thus modified. I therefore believe that Darwin's theory of subsidence will regain in this century the general acceptance it enjoyed through the middle of the last; and that the several alternative theories, which have found more or less favor in the last thirty or forty years, will be given minor rank or discarded altogether.

Retrospect.—The work of four men was often in mind during my Pacific voyage: Darwin and Dana of an earlier generation, Agassiz and Shaler of our own times. Nathaniel Southgate Shaler, my first teacher in geology, always my best friend at Harvard, the man who opened opportunity to me, who gave me encouragement and support when both were much needed, a man of ever-widening relations with his fellows,

in whose memory my journey across the Pacific is only one of many journeys that will be undertaken as the years pass. Alexander Agassiz, a man of varied and vast achievement, who saw and studied a greater number of coral reefs than any other scientific observer, and who gave all of us the great example of working untiringly to the end; but a man of so reserved a disposition that few of his associates knew him intimately. It is always a regret that my acquaintance with him was slight, and still more that I can not accept the conclusions to which he was led by his many journeys in coral seas.

Darwin and Dana, explorers of the Pacific nearly a century ago. How deep is the impress of these two men on the coral-reef problem! How inevitable is the mention of their names whenever coral reefs are discussed! But we must not picture the Darwin of coral-reef fame as a venerable old man, whose face, although wearied by work through long years of ill health, was always so patient and benign; nor yet as a man of middle age, who, steadfast against a flood of prejudiced criticism, opened new fields for thought and changed the philosophy of an unwilling world; but as a diffident youth, who, when some one was wanted to do scientific work on a long sea voyage eighty years ago, offered his inexperienced services for what they were worth, and yet carried with him such a fund of original thought that, while still in South America, he invented the best of all coral-reef theories before he had ever seen a true coral reef. And the Dana, who, younger than Darwin by four years to a day, followed him across the oceans, was not the grand figure who was still with us, so bravely alert in well-conserved erectitude, hardly more than a score of years ago; nor yet the man in middle life who, setting us all a measure of industrious versatility that none can now attain, standardized three branches of science in America; but a young man, little taught, yet studious and observant, who standing thoughtful on a mountain peak not in Darien but Tahiti, first learned a deep secret of the Pacific, and added to Darwin's theory of coral reefs the confirmation that even Darwin himself overlooked!

The Pacific still has many prizes for the observant explorer. Let the student who would discover some of them note the good example set by Darwin and Dana, and cross the great ocean early in his life, while he is yet young enough to have, after his return from its inspiration, many years for work; but if he finds it impossible to go early, better late than never!